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ming Water, in Seas, and in Clouds, and in Pickle ; yet not so frequent, as to escape always the suspicion of being Prodigies. But in the foresaid references more is said of Light, than I am able to express ; I shall only add, That I gave full warning to observe, whether the Light in my two Instances had any blewish or greenish tincture ; all that saw both, affirmed the Light to be as clear as the brightest Moon-shine, and so it appeared to my own eyes ; and I can perfectly remember, that I really thought the beams which came from the Mackrel, and the stirred pickle, to be bright Moon-shine, till a Servant brought me to the Vessel, to see the contrary.

Postscript. We had the report here (whether true or false, you may best know) of the shining Beef in the *Strand*, about the same time, when the Neck of Veal, first mention'd, shined here. And it was here observed, That the Stars had that night a glaring brightness and largeness, more than ordinary, and for some months before, and ever since, the weather hath been more gentle, warm, and dry, than is usual in those months ; but 'tis above my skill to demonstrate, how this belongs to the matter in hand. Note, that the Mackrel-pickle was thick and not transparent, till it was stirred and flaming ; the Pork-pickle was clear, or transparent, yet shined not in any part.

A Discourse concerning the Spiral, instead of the supposed Annular, structure of the Fibres of the Intestins ; discover'd and shewn by the Learn'd and Inquisitive Dr. William Cole to the R. Society.

DISCOURSING (near two years since) with a very ingenious Person, concerning the Mechanical reason of the *Peristaltick* motion of the Intestines, which is by Anatomists deduced principally from *Annular* fibres, constituting, according to the received doctrine (with the right fibres immediately investing them, though, by the by, I take these to make a distinct coat) one of the coats of them ; his sence was (which he told me was that likewise of some others of his acquaintance) that they might be rather numerous, though small, Sphincter-muscles, than single fibres, to which that motion is to be attributed ; Muscles being in most, if not all, other instances owned to be the adequate instruments of motions analogous to this ; and fibres, though absolutely necessary, yet being no otherwise so, than as (a number of them being collected, and fitly disposed) they constitute a Muscle.

The Conjecture seemed to me more probable than the vulgarly received

received opinion: but yet (with all respect to the abettors of either) several difficulties occurred to me, whether of the two suppositions soever were allowed.

For, *first*, I conceived it might be doubted (each of these, whether single fibres, or muscles, being supposed distinct, as I think they generally are, and, if annular, I conceive, must be) how the actuating matter, or impression (according to the opinion of some learned men) should be transmitted from one to another down along the whole tract of the Intestines; since Nature's usual way, for the propagation of Animal motion, is by a Continuation of vessels, (or at least fibres, whether they be concave or not) from the part where it begins to that to which 'tis imparted, either for the conveyance of some actuating substance, or (according to the other *Hypothesis*) the communicating an impression. But there being, in the *Annular* supposition, no such continuation of vessels or fibres, a lateral contiguity being all that can be pretended, it might perhaps be urged, that the influent and moving matter (according to that notion) might be transmitted by mutual inosculations between the contiguous fibres along their sides; which, if there be no Communication by vessels, was the only way, I could guess at, to solve the doubt; for, the notion of an *Impression* would hardly do the business, since it seemed not evident, that there could be, in that supposition of a Continuity of fibres, tenacity enough in the Intestines to carry on such a motion. But to this I considered,

Secondly, That such a supposition seemed not very agreeable to Nature's methods, which ordinarily makes use of Vessels (and those both close, and as direct as the design and organization of the part will bear,) for the transmission of the fluid substances in the bodies of animals, not lateral emissaries; except where some great inconvenience is designed to be prevented by the help of such conveyances; as, for instance, by the Anastomoses, discovered to be between veins and veins, arteries and arteries, in which vessels the blood running with a large and rapid stream, should any of them chance to be obstructed, the Circulation, so necessary to life, must needs be intercepted, without some lateral conveyance of it into others of the same kind: Which inconvenience yet I supposed would hardly be alledged in the present case; *that* fabrick of those vessels seeming to be designed for extraordinary emergencies, but *these* being, according to the present supposition, the constant and necessary ducts of this actuating matter. But nevertheless,

Thirdly,

Thirdly, It seemed difficult (to me at least) to solve this Intestinal contraction, though these lateral apertures were supposed: For, if fibres (whether considered as single, or as constituting a muscle) be contracted according to their length from some influent matter, it must be (according to my sense) from a distension of them in breadth; and, in order to that, this matter must undergo some confinement in the part to be distended; but if they have lateral perforations (and those in the opposite part proportionate to those in that which admits this matter, which must, I conceive, be granted, since the contraction is all along the Intestines proportionate,) how can it be supposed, a distension (at least such a one as is here required) can happen, when the matter designed to effect it has so ready a passage forth, especially its determination from the impelling cause being in right lines downward? If it were objected, that the motion of this substance might be supposed to be lateral as well as direct, in regard there would be a passage for it into the fibres as well as through the Anastomoses, and that in proportion larger than through these, whence nothing seems to hinder but that a distension of them might follow; I supposed, it might be replied, that, by reason of such a distortion of part of the impelled matter, it seems, that the impressed motion would be soon lost (according to the laws of motion) unless the impelling cause were more violent than I see reason in this case to imagine it to be. But indeed I think, no Anatomists have observed, that muscles (supposing these such) receive their actuating matter in at their sides, or, when their motion ceases, send it forth that way; but all, so far as has been observed, are fenced with a considerably compact, and (comparatively) impervious membrane.

Fourthly, I considered, that all muscles are observed to have two tendons, one at each extremity, by the approach of one whereof toward the other, its motion, which is contraction, is performed; but it seems hard to conceive, that these tendons should coincide (as in this supposition they must) and, if they do, I presumed it would be difficult to determine, what part of these circular muscles (if such) the tendons are, and where the motion should begin in each; it being observed, that all muscles are fastned to some, either simply or comparatively, unmovable part, toward which (ordinarily) they move, and by which the instinct of motion is from the nerves conveyed to them: But no Anatomists, (so far as I had observed) having discovered, that any one part of these

these muscles, or moving *fibres*, whichsoever they be, has any stricter cohesion than other with any of the adjacent parts, I conceived, I might be allow'd the liberty to doubt of the *Hypothesis*, especially if I could satisfy myself better by another.

For instead of these solutions there occurred to my thoughts a third way, which (provided experience would countenance it) seemed more mechanically adjusted to solve the *Phænomenon*; viz. That those *fibres*, which have been esteemed *annular*, might perhaps be *spiral*, and so be continued down in one tract to the lowest extremity of the intestines; withal, that their smallness, compared with the compass they fetch about the intestine, might very easily, I conceived, impose upon any, who made not those reflections, or tried not to unravel them; their declination being, for that reason, not easily discernible: Which if true, it seemed probable to me, that when either a bare motion shall be impressed on them at their beginning, or any substance impelled into them, they being to be supposed *in statu naturali* moderately tense, so long as the moving cause continues, the motion must be successively continued all along their tracts, and, that being in *ambitum*, must therefore, whilst it lasts, by abbreviating these *fibres*, straiten the intestine, and so thrust forward what is contained in it, especially if they proved to have a muscular fabrick. The conjecture as 'twas not disrelished by the person to whom I proposed it, so gratified me the more for the seeming easiness of the performance; Nature's operations being the most easy and simple that can be imagined, though for that reason very often, I doubt, overlook'd. But the notion lay afterward long *dormant*, till, about half a year since, being revived by I know not what occasion, I consider'd 'twas too unphilosophical to acquiesce in bare speculation, when *autopsy* might be consulted; and therefore I set upon the experiment, which I first made in a portion in the upper intestines of an Ox, which, by reason of their largeness of proportion to those of most other *species* of animals, seem'd fittest for the tryal; afterwards in those of Sheep and Calves, beside the repetition of it in Oxen, and not only in the smaller intestines, but in the *colon* and *cæcum* also. The circumstances and result of which tryals are as follows.

To effect a due disjunction of the membranes and *fibres* (which I found 'twas hard, if not impossible, for me to make while 'twas raw,) I was fain to cause the intestine of Oxen to be boiled 5 or 6 hours, of Sheep 4; whereby the compages of the parts was so loosned,

loosened, that the two outward coats, *viz.* the common one, and that consisting of right *fibres* were easily separated (if it were attempted soon after it was taken out of the water) from that to which my search was destined, and left those reputed *annular* ones naked; (though, by the way, too long coction would prove prejudicial on the other hand, by too much intenerating the *fibres*.) These at the top of the intestine I attempted to separate from one another; and when those, which had been decurtated by the unequal cutting of the knife, were taken off, I found,

First, that I could not separate a single *fibre* from his fellows to any considerable distance, all of them (to my observation) being very small, and in the separation running smaller and smaller, and withal by reason of their implication or stricter cohesion one with another easily breaking; but a congeries of them (to be observed especially, though not precisely alwaies, in those places, where by gently extending the intestines several times, and then letting it return again, the cohesion of the several *series* of them became loosened) which at first view would resemble a pretty large *fibre*, would without much difficulty rise together; the very small constituting *fibres* of which clusters yet, if the boiling had been very long continued, whereby the compages was very much relaxed, would in the raising be very apt to separate from one another, and appear distinct, by reason of their insertions, by and by to be mentioned.

Secondly, that when, beginning at the top, I attempted the separation of one of these (supposed *annular*) clusters of *fibres* towards my right hand (on that side of the intestine, I mean, which was turned towards me) a whole ring would come off together, (excepting that some *fibrillæ*, which, rising from contrary parts, decussated one another at the top in that *phasis*, would a little retain it) which at first stagger'd me as to my forementioned conjecture; but endeavouring it towards my left, I found, for the most part, I could easily enough unravel that cluster to a considerable length, *viz.* that of sometimes more than two or three spans, before rupture (of the whole cluster I mean,) which yet at last 'twould be subject to. For,

Thirdly, though those convolutions, as to the greatest part of them appeared distinct, yet I found, that from every one of them at short distances some *fibres* did obliquely, and the most of them, to my best observation, according to the course of those I have mentioned, insert themselves into the next convolution, and become a part of it; though withal some I observed to have a contrary ten-

dency, or rather seem'd to ascend from the lower to the upper convolution, and help to constitute it, and so to observe the course mentioned; nay, sometimes would go farther than the next convolution, and, running under it, apply themselves obliquely to some higher, which yet being in a smaller number than the rest that lay in the order contrary to them, did not very much hinder the diffociation of the main ones: which *fibres* breaking off, and that in some places in greater numbers than in others, would at last (and the sooner if the intestine began to grow dry, which 'twould quickly do) cause the whole cluster to break off.

Fourthly, I observed, that as the most of these *fibres* would by degrees according to the order of the convolutions, insert themselves into the next, so some of them would (in the same order) pass over it, and more (so far as I have observed) would run under it, and either adjoin themselves to some more remote, or elude my searching by hiding themselves under them. This insertion of these *fibres* seems to be the reason of the *annular phasis*, that I mentioned even now, in the contrary way of separation: For, the attempting it contrary to their order, must hinder in some measure the ready diffociation of the next convolutions upwards; especially near the severed extremity, where there is less resistance of the adjacent parts; the mentioned *fibres* also seeming somewhat bigger, and consequently stronger, in the upper, than after their insertion into the lower convolution: Though indeed

Fifthly, I found, that if I began at a lower part of the intestine, and try'd to unravel *upwards*, there was not much more difficulty in so doing, than when beginning above, I attempted it *downwards*; of which the reason, I suppose, might be the tenderness of the part occasioned by long boiling, whereby I could not perhaps judg of the degrees of renitency in those small *fibres*. In this contrary way of separation too, the operation, I observ'd, would not succeed, unless I attempted it in the contrary order, *viz.* towards my right hand.

Sixthly, when before boiling I caus'd the inside of the intestine to be turned outward, as I did in two tryals, and afterward by taking off the glandulous and vascular coats (which I think to be distinct from one another, as I said before of those consisting of right *fibres*, and the supposed *annular* ones,) endeavour'd to unravel the *fibres*, I found they would come off in the contrary order, *viz.* from my left hand toward my right; which, I conceive, confirms the observation above deliver'd, in regard the intestine being inverted, the order of separation must be so too; though I found

found(or thought) the operation more difficult, by reason of some *fibres* lying in the opposite order (mentioned under the third particular) and in this appearance lying uppermost.

Seventhly, in one of these attempts of unravelling the *fibres* of the intestine of an Ox, so inverted, I found, that though the *fibres* I took up came off in the order I just now mentioned, yet running over some others, they made a more oblique excursion, and for two or three convolutions left betwixt them a considerable *area* of *fibres*, amounting (according to my conjecture) to five or six times, or more, the breadth of those that so came off, till going deeper and deeper among the other *fibres*, and at last running under them, they could be no longer traced, but brake off. Whether this be usual, or only *lusus naturæ*, I cannot determine.

Eighthly, I found it much more difficult (in that one tryal I made) to unravel the *fibres* of the *Cæcum*, than the other intestine, which seemed more interwoven than those of the rest, and to have contrary tendencies one among another.

This is the sum of my observations hitherto concerning this coat, which I take leave to think one concave and Helical muscle (if I may so style it:) And that it might be supposed such, the fore-mentioned insertions seem'd to evidence, they appearing to me in the separating appositely enough to represent the fabrick of a muscle delivered by the accurate *Steno*. Where the tendons of it are fixed, is not evident; but, if I may have the liberty to conjecture, I should think the upper of them to be radicated (at least) at the *pylorus* (if not as high as the *sphincter gulæ* (if this be not it,) since, the carneous coat of the stomach being by the Learned Dr. *Willis* found to be a muscular texture, and there being a continuation of motion between that part and the intestines, it seems to me not altogether improbable they may be but one muscle; and the other at the *anus*.

Whether the supposed *annular fibres* of the *veins* and *arteries* may not have the same fabrick as those of the *Intestines*, since both these kinds of vessels seem to have a *peristaltick* contraction of their own, and not to be bare conduit-pipes to transmit the impelled blood, I propose to be considered and examined by persons of more acute hands and judgment; as I do all what I have here delivered, not daring too much to trust even the informations of my own hands and eyes, till I find them confirmed by those of others, more judicious as well as dextrous in making experiments.